experiments with the object of determining more definitely the properties and chemical behaviour of this elusive parent. The general manner of proceeding has been to obtain as complete a solution as possible of known weights of different uranium minerals. These solutions have been treated in the manner described in my earlier communication, with special precautions and modifications. The growth of radium in the solutions of the rare earths finally obtained was determined by measurements of the amount of radium emanation present at frequent intervals, and the rate of growth was calculated by an expression which took into account the rate of production of the emanation by the radium. The minerals used included carnotite, Joachimsthal pitchblende, gummite, uranophane, and a specimen of very pure uraninite from North Carolina containing only 0·03 per cent. of material insoluble in dilute nitric acid.

The space available in these columns will permit of only a brief mention of some of the more interesting results. In confirmation of Rutherford's statement it was found that the rate of production of radium was not influenced appreciably by the presence of radio-actinium and its products, which were completely absent from most of the solutions at the start. Continued observations of the growth of radium in the first solution prepared indicate that the rate of production of radium has been constant, within the limits of experimental error, for a period of more than 500 days.

I have attempted with one of my preparations to repeat the separation of the radium parent from actinium by the ammonium sulphide treatment which Rutherford has described. No separation could be detected when freshly prepared, pure ammonium sulphide was used. It was found that the radium parent can be quite completely separated from actinium by repeated precipitation with sodium thiosulphate under the conditions usual for the precipitation of thorium. In the case of a solution of the parent substance with thorium and other rare earths treated in this manner, less than 1 per cent. of the parent present remained in the filtrate, as was shown by the growth of radium in the two fractions obtained in this process. Since ammonium sulphide is always open to suspicion unless freshly prepared, and since on standing in loosely stoppered bottles it ultimately changes wholly into ammonium thiosulphate, it appears probable that the separation noticed by Rutherford was due to the latter reagent.

An interesting relation has been noticed between the growth of radium and the activity of the substances other than thorium in my solutions containing the radium parent. This proportionality is quite striking in those solutions containing the more completely purified salts. The activity of the substance present in these salts is comparatively high, and is about equal to the activity of the radium (itself) with which it is associated in the mineral. More significant still is the fact that this radioactive substance does not appear to possess any of the characteristic properties of the recognised radio-active elements. It is impossible that it is uranium, thorium, radium, or polonium. It has none of the properties that have been given as characteristic of actinium. About four-tenths of a gram of thorium oxide, containing an amount of this new body sufficient to give a leak of 500 divisions per minute in an α-ray electroscope, did not produce sufficient actinium emanation to permit its detection in another electroscope of greater sensitiveness. The thorium oxide had been prepared some weeks before by the gentle ignition of the oxalate, and was very porous. A strong current of air, about four litres per minute, was drawn over the preparation. There was no difficulty in measuring the thorium emanation evolved by this material under these conditions.

That the active substance is not actinium is also indicated by the fact that from a solution more than five months old no active substances other than thorium products could be separated by treatment with ammonia, by the formation of finely divided sulphur from sodium thiosulphate, or by the precipitation of considerable quantities of barium sulphate in the solution. The first process should have separated actinium X, and the two last should have separated radio-actinium had these products been present. The solution contained about 3 grams of thorium and a quantity of the new substance having an activity equal to that of about 35 grams of pure uranium.

Another important matter is the behaviour of the oxides obtained by strongly igniting the hydroxides precipitated by ammonia from a solution similar to the above. The activity of these oxides remains nearly constant for long periods, showing only a slight initial rise corresponding to the formation of thorium X in the thorium present. No rise corresponding to the formation of actinium X can be observed, but if actinium were present a separation of this product would be expected.

For these and certain other reasons I think that there is good cause for believing that uranium minerals contain an element emitting α rays, which is different from the other elements that have been identified, which produces no emanation, and which resembles thorium in its chemical properties. The activity of this element appears to be about the same as that of the radium (itself) with which it is associated in minerals. It is without doubt a product of uranium, and is probably the immediate parent of radium. It is very likely that this body is contained in Debierne's actinium preparations and in Giesel's "emanium" compounds, especially in the former, and its presence may perhaps explain the confusion which has resulted from Debierne's earlier assertions that actinium accompanied thorium as opposed to Giesel's positive statements to the contrary (*Chem. Berichte*, xl., 3011). The proportion of the total activity of a mineral due to the actinium present is very small, for the activity which can be attributed to actinium is less than 9 per cent. of the total.

The rate of disintegration of radium as determined from its growth in preparations similar to those described above, separated with great care from very pure North Carolina uraninite, indicates that the half-value period of this element is about 1900 years. It is hoped that certain experiments now in progress will make it possible to determine this factor with a satisfactory degree of certainty.

BERTRAM B. BOLTWOOD.
Sloane Laboratory, Yale University, New Haven, Conn., September 9.

The Body of Queen Tii.

IN NATURE of September 12, p. 494, a summary description was given of the remarkable discovery made by Mr. Theodore M. Davis, of Newport, R.I., of the tomb of the famous Egyptian Queen Tii, Thyi, or Teie, mother of the heretic-king Akhenaten, at Thebes. A remarkable point with regard to this discovery has been raised by an "Occasional Correspondent" of the *Times*, who informs us that the supposed remains of the queen, after having been examined by Dr. Elliot Smith, turn out to be those of a young man, at most twenty-five years of age! It is concluded therefrom that the discoverers were mistaken in their attribution of these remains, and that the coffin is not that of the queen at all, but of Akhenaten, whose name appears on it; but this cannot be the case. On the catafalque the inscription definitely states that it was given by Akhenaten to his mother Tii, and the mention of Akhenaten's name only on the coffin need mean also no more than this. The coffin is that of a queen; the diadem and necklace and other objects found are also the parure of a queen, not of a king, and the heads of the canopic jars are portraits of Tii.

The fact that the body found with these things is that of a man would mean simply that, as Prof. Sayce says in a letter on the subject published in the *Times* of September 17, "the mummy of the Queen had been torn to pieces like that of the King; and that, subsequently, when an attempt was made to put the tomb in order, the first mummy that came to hand was thrust into the Queen's jewelry wrappings, and coffin. It was not the first time that the Egyptians resorted to similar measures, and it would explain the otherwise puzzling absence of funeral furniture in the tomb."

In an article published in the *Graphic* of September 14 describing the tomb, I assumed that the weight of Dr. Elliot Smith's medical authority was decisive, and that therefore the body must be regarded as that of a man,

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at the same time suggesting reasons for this fact more or less similar to those advanced by Prof. Sayce; but the discoverer is by no means convinced that Dr. Smith is right at all. Since penning my article in the *Graphic* I have received a letter from Mr. Davis, giving his reasons for his sturdy belief, in the same terms as one received by Prof. Sayce, who communicates its gist to the *Times* as follows:—"Immediately after the opening of the tomb he had the mummy examined by Dr. Pollock, of Luxor, 'and a prominent American obstetrician. . . . In the presence of the doctor and surgeon, Mr. Ayrton, and one or two other persons, the mummy was opened and the bones exposed. In fact, the mummy had absorbed so much moisture that it could not be unwrapped, but it yielded to the touch and disintegrated to such an extent that there was no difficulty in exposing the bones from end to end. The pelvis was admitted to be the criterion of the sex. Both doctor and surgeon instantly agreed that it was the pelvis of a woman. The surgeon made a most thorough examination, and explained to us why it was a woman's pelvis, and what the difference is between the pelvis of a man and a woman. He practically stated that the 'greater breadth of the pelvic arch gives one of the most easily appreciable points of contrast between the male and female pelvis; the pelvic arch in the female forms an angle of from 90° to 100°, while in the male it averages from 70° to 75°.'" To me Mr. Davis writes:—"In any event, I shall exhaust the question of the sex of the pelvis before conceding Dr. Smith's opinion."

If, however, Dr. Elliot Smith is really right, and the body is that of a man, the fact does not in any way affect the discovery. The tomb, catafalque, coffin, canopic jars, diadem, &c., are those of Tii, and the bones found were in some way substituted for hers. They cannot be those of Akhenaten, as he must have been a middle-aged man when he died. H. R. HALL.

Use of the Word "Telephotography."

NATURE of August 29 contained an article by Dr. Shelford Bidwell entitled "Practical Telephotography." May I enter a protest against the use of the word "telephotography" to describe the method of transmitting pictures to a distance? Without wishing to go into the merit of the term, I would point out that it has been applied for years to photography by means of a lens consisting of a negative as well as a positive element, as in the well-known "telephoto" lens of Dallmeyer. There is already an extensive literature in which the term "telephotography" is used with this meaning, and to employ it now to describe something totally different can only ultimately result in confusion. Would it not be better to employ the term customarily employed, viz. "phototelegraphy"? R. CHILD BAYLEY.

20 Tudor Street, London, September 7.

I BELIEVE that the word "telephotography" was coined by myself, and first appeared at the head of an article published in NATURE on February 10, 1881, in which an account was given of the earliest attempt to transmit photographic pictures by electrical means. The term was at the time generally adopted by the Press, and has found its way into several books of reference. In the "Century Dictionary" (1900) telephotography is defined as "the art (not yet attained) of producing a photograph, distant and invisible from the camera, by means of electrical connections with a suitable apparatus near the object." No other meaning is given.

The word was not employed in the sense in which it appears to be now current among photographers until at least ten years later, the date of Mr. Dallmeyer's invention being 1891; but I have no great affection for my neologism, and propose in future to write "telegraphic photography," thus avoiding the possibility of confusion. "Phototelegraphy" I take to mean signalling by flashes of light, as in heliography. SHELFORD BIDWELL.

Beechmead, Oatlands Chase, Weybridge, September 13.

DOUBLE STARS.[1]

PROF. BURNHAM tells us in the preface to the first mentioned of these works that when he was attracted to the subject of double stars he had to draw the main part of his information from an early edition of Webb's "Celestial Objects." A useful book, no doubt, and one which has given many an amateur his first insight into a fascinating study, but its modest dimensions compared with those of the ponderous tomes the titles of which appear at the foot of this column may serve to remind us of the progress that has been made since that book occupied a prominent place as a recognised authority. It is in no small measure due to the difficulties arising from this scantiness of information that Prof. Burnham experienced in his early days that we owe this magnificent compilation. In those far-away times it was necessary to collect the history of double stars, to make manuscript copies of all the catalogues that could be obtained, to note carefully all that was published; and though this necessity may no longer exist, these manuscript catalogues have been kept posted up to date, and it is the final and complete outcome of this long-continued work that has now found its way into the printers' hands. We therefore get the results of accumulated experience in the form that the author has found most useful.

The catalogue gives the approximate coordinates of 13,665 stars, the position angle and distance at a given epoch, the magnitude of the components, and such other information as can be pressed into a single line. The value of such a work consists in its completeness. It may be confidently assumed that some information concerning every star recognised as double within the area under review previous to 1906, will be found here. All who have worked with incomplete or disconnected catalogues will know how to appreciate the usefulness of this compilation. In no department of astronomical research is the literature more scattered. Amateurs have contributed much to double-star measurement, and their observations are necessarily distributed through many channels. To collect and make available these many sources of information is a task of no common difficulty, and is perhaps only possible to one who has narrowly watched the growth of the material and sifted the details as the observations appeared. Alertness, industry and a keen interest in the subject were as necessary as access to publications or orderly method of arrangement. No one was better equipped for the task than Prof. Burnham, and we may be grateful that he has accomplished it.

The notes to the catalogue will be of greater interest to those who are concerned in the attainment of astronomical results than is the catalogue itself. Here are recorded a sufficient number of measures to show the motion where there has been any relative change, and so far as possible its character and amount, or to exhibit the unchanged relation of the components where no motion has been detected. References to the original places of publication, which would be used in subsequent calculations, are given for each star. The author gratefully acknowledges that he has been given a free hand in the selection of observations and comments, and he adds that he "has omitted nothing that in his judgment would be worth giving." Here the author assumes the position of a critic, a position for which he is admirably fitted by his long training and close study. Many will be prepared to surrender their judgment

1 "A General Catalogue of Double Stars within 121° of the North Pole." By S. W. Burnham. Pp. lv+256. (Washington, D.C.: Published by the Carnegie Institution, 1906.)

"A General Catalogue of Double Stars." Part ii. Notes to the Catalogue. Pp. viii+257–1086. (Published by the Carnegie Institution, 1906.)

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